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TWO ESSAYS IN THE PHILOSOPHY OF ECONOMICS

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ABSTRACT

This paper addresses the question of how fundamental theories in economics (e.g. game theory and general equilibrium theories) are related to theories of specific markets or market phenomena. This question arises because, in neoclassical economics, fundamental theories typically are not subjected directly to empirical tests. The argument presented here has two parts. First it is argued that even without being directly tested itself, fundamental theory can help to refine, test and evaluate specific theories. Six ways in which this can occur are enumerated. Second, it is argued that the close relationship between fundamental and specific theories is a general feature of positive economics rather than being a special feature of neoclassical theory, and that this relationship makes it possible systematically to test or evaluate fundamental theory. Views of Friedman, Machlup and von Mises are considered.

ON THE ROLE OF FUNDAMENTAL THEORY IN POSITIVE ECONOMICS

Edward J. Green

1. INTRODUCTION

There is a standard distinction in economics between positive theory, which concerns law-like propositions about individual or aggregate economic behavior, and normative or welfare theory, which concerns the evaluation of alternative policies or institutions. In addition, contributions to economic theory are often classified according to whether they are "pure" or "applied." Although this classification is not a precise distinction, there is substantial agreement about which contributions are quite "pure" and which are rather "applied." The present paper concerns the relation between positive theories which lie toward opposite ends of this spectrum. In order to avoid some misleading implications of the traditional way of speaking, I will employ the term "fundamental theory" to refer to contributions which are usually called "pure," and "specific theory" to describe those which are usually called "applied." Although I will offer definitions of these terms, I do not intend to posit a rigid distinction, of which every contribution must fall unambiguously on either one side or the other. Another understanding which should be explicit at the outset is that, in discussing the role of fundamental theory, I will be systematically vague about the philosophical theory of science. My point will be that fundamental theory performs functions and satisfies criteria which would be necessary on any reasonable view of science, rather than that some particular view can explain its

existence and justify its usefulness. In particular, when I use terms like "empirically significant" and "research program," I intend these terms to be taken in a general way rather than in the technical senses which they have acquired in specific philosophical theories.

Specific theories in positive economics are easy to recognize as scientific theories. In these specific theories, models of particular markets are formulated, and empirically significant consequences are derived. Questions like why these consequences are significant (e.g., that they are verifiable or refutable) may be matters of debate, but these are questions about science in general and not questions about the scientific status of economics.

The parallel between fundamental economic theories and theories in the physical sciences is not so clear. Fundamental theories typically look like arguments from first principles to propositions devoid of empirical content: for instance, that a competitive equilibrium exists. Even within the economics profession, many have profound doubts about the value of such exercises. Concerning the existence of equilibrium in particular, many economists would subscribe to the view that "questions of nonexistent equilibria are virtually irrelevant. Clearly something is happening in the real world. The object of science is to explain these events. Dwelling on the possible nonexistence of these events is of questionable empirical value." [10, pp. 526-527] Even the terminology of "pure" versus "applied" theory seems to carry the implication that the former may be an essentially speculative endeavor.

That this implication is false, and that fundamental and specific economic theories are really complementary, is the thesis of

this paper. The argument has two parts. First, fundamental theory is defined, and an account is given of how, in neoclassical positive economics, fundamental theory enhances the acceptability of specific theory as a research program. This account should establish that fundamental theory cannot be isolated from specific theory as easily as is implied by the skeptical view quoted above. However, the account might be turned to charge that neoclassical specific theories are unacceptable because they depend on "unscientific" fundamental theory, rather than to argue that fundamental theory is "scientific" because it is of a piece with specific theory. The second part of the argument here will forestall this move by pointing out that reliance on fundamental theory is not a peculiar feature of neoclassical economics, and by suggesting that standard views of science indicate ways in which fundamental theories can be evaluated systematically. The second part of the argument will reinforce the first part by establishing that both fundamental theories and specific theories must be included in any viable research program in economics, whether neoclassical or other.

2. THE USEFULNESS OF FUNDAMENTAL THEORY TO NEOCLASSICAL POSITIVE ECONOMICS

While specific theory has already been characterized in the introduction of this paper as the formulation and study of models of particular markets or market phenomena, fundamental theory has been described only as not being specific. In fact, it is typical for fundamental theory to be described either in this way, or else by enumeration of some fundamental theories, or else according to some

ad hoc criterion (e.g., that fundamental theory uses advanced mathematics). In view of the lack of a clear conception of what fundamental theory is, it is not surprising that there is uncertainty about how it is related to specific theory or whether it is related at all. A definition of fundamental theory will be offered now, and some ways in which fundamental theory contributes to the progress of specific theory in neoclassical positive economics will be discussed.

I will define a fundamental economic theory to be a body of propositions which describe, in general terms, the relation between institutional structure and individual behavior. This notion of fundamental theory is similar to the idea of equilibrium theory proposed by Daniel Hausman [5]. The most important difference is that, while Hausman conceives of equilibrium theory as a determinate, if implicit, body of propositions, I take fundamental theory to be a family of explicit theories which are distinct though closely related. For instance, one version of competitive theory might state that there are many firms with small efficient scales of production, while another might assume that each firm has a convex set of production possibilities but make no assumption about the number of firms. Each of these versions entails that the set of aggregate production possibilities for the economy is convex, which is an important condition for existence of equilibrium, and each covers some situations which the other does not.

As a rule, fundamental theories in neoclassical economics consider explicitly the optimization problems of all agents studied. That is, institutional structure is represented as a specification of

these problems, and individual behavior of an agent is represented as optimal solution of a problem relative to some exogenously specified objective (i.e. tastes or profit maximization). Paradigmatic examples of fundamental theory are the theory of games (e.g. [9]) and the theory of general equilibrium (e.g. [3], [13]). In general equilibrium theory, for instance, consumers' demand (individual behavior) determines the market-clearing price (institutional structure), and the price determines consumers' budget sets in terms of which their demand is defined.

In contrast to specific theories, it is often difficult to state in concrete terms why a fundamental theory is of interest. For example, the response of a fundamental theorist to an inquiry about his particular field of study might be, "I study the difference between monetary and barter economies." This theorist presumably has no intention of conferring with an anthropologist when his research is done, to find out whether there is an example of a pure barter economy which refutes or corroborates his theory. Rather, he may be trying to formulate reasonable conjectures about functions called "demand for money" and "utility of money holdings" which appear in monetary theory at a more applied level. His hope is that, when the specific theory is augmented by these conjectures, it will be a better scientific theory in terms of predictive power, refutability, and so forth. Viewed in this light, the fundamental theorist's work seems much more oriented toward application than it did at first glance. His initial description of his work suggests a speculative concern with exotic or non-existent economies, but his actual concern is quite down to earth.

The foregoing example illustrates two facts about fundamental theory in economics. First, the distinction between fundamental theory and specific theory cannot be based on the practical orientation of the latter. It is not the case that fundamental theorists are interested in a different set of phenomena from their colleagues. Second, fundamental theory is not an alternative research program to specific theory. In particular, the formulation by fundamental theorists of formal systems (e.g., the Arrow-Debreu [3] model of general equilibrium) which differ markedly from those used in specific theory does not necessarily signal an effort to refute specific theory, or to compete with it in any way.

Fundamental theories may, as Hausman suggests in [5], serve an explanatory role of a special kind. However, this role does not exhaust their usefulness. In particular, fundamental theories can be used to help refine, test and evaluate specific theories. Six ways in which this may occur will now be enumerated.

First, fundamental theory in some instances extends the domain of application of specific theory. One example of this phenomenon is Arrow's [1] theory of state-contingent securities which has (among other applications) advanced the study of financial markets by providing a tractable representation of complicated and diverse portfolios of financial assets. Another example is Lancaster's [6] theory of hedonic pricing, which provides a basis for the study of markets in which there is substantial product differentiation (e.g., automobiles).

Second, as in the example of the introduction, fundamental

theory sometimes suggests restrictions on the parameters of a specific theory which enhance the testability and predictive power of the latter. The best-known example of this is Slutsky's [11] characterization of demand functions which arise from utility-maximization. This characterization provides useful restrictions on the functional form of parametric demand functions for econometric estimation.

Third, fundamental theory provides a unified description of phenomena which are observed in diverse specific fields. For instance, there are convexity and continuity conditions which are sufficient both in game theory and in general equilibrium theory for existence of an equilibrium. If he can show that these conditions are met, a specific theorist is assured that he is working with a logically consistent set of assumptions. Moreover, in markets which behave pathologically (e.g., "natural monopolies" where competition is technologically impossible to sustain), investigation of precisely how conditions for the existence and efficiency of competitive equilibrium fail is an informative diagnostic exercise. Such investigation sometimes leads to realization that apparently special features of diverse markets are in fact instances of a single phenomenon which can be systematically explained. A current attempt to unify such phenomena is the theory of signalling initiated by Spence [12]. This theory explains a number of instances of seemingly irrational market behavior (e.g., setting of educational requirements for job applicants which are unrelated to their prospective duties, payment by firms of dividends although earnings could be distributed by other means which receive more favorable tax treatment) in terms of the unobservability

of quality differences among goods sold in a single market.

Fourth, fundamental theory reconciles specific theories which apparently conflict, by distinguishing between their domains of application. Typically, several specific theories are embedded in a single general theory in such a way that the original theories are recoverable by imposing special conditions on parameters. The classical case of such a reconciliation is Cournot's [2] embedding of the theories of monopoly and perfect competition in a game-theoretic theory of non-cooperative industry equilibrium. This general theory foreclosed the objection that refusal to consider industries with few firms as counterexamples to the theory of perfect competition, or refusal to consider industries with many firms as counterexamples to the theory of monopoly, constitutes an ad hoc avoidance of refutation.

Fifth, fundamental theory helps to isolate tests between conflicting specific theories by facilitating precise analytical comparisons between them. For example, there is a long-standing controversy in the applied theory of the firm concerning whether managers try to maximize profits or share value. Using the theory of state-contingent securities, it may be shown that these two objectives coincide for a manager if investors can use assets of other firms to hedge their investments in the firm in question. Armed with this result, empirical economists are able systematically to avoid trying to analyze data which could not possibly discriminate between the two hypotheses.

Sixth, fundamental theory provides informal criteria of coherence for specific theory. By this is meant something akin to the

"negative heuristic" described by Lakatos. Consider the case of Malthusian income-distribution theory. According to this theory, the working class population would grow until it was constrained by the imminent threat of famine, while capitalists would limit their family size in order to allocate some of their resources to saving and investment so that their consumption could be maintained at a comfortable level. This description of affairs was supposed to be true in the long run, without workers ever learning from observation of the capitalists how to better their lot. In order to express the Malthusian law within a theory of utility maximization, it must be assumed that workers possess an insatiable sexual appetite or a fanatical desire to have large families, while capitalists have epicurean tastes. Such an attribution of preferences to economic agents would be formally consistent, but it is incredible. As a matter of scientific common sense, this theory would be removed from consideration. If applied theory were formulated by placing hypotheses directly on the behavior of agents, the Malthusian theory would not be open to any objection on methodological grounds. This regulative use of utility-maximization seems to have an analogue in physics, where an empirically successful applied theory would be viewed with suspicion if intellectual contortions were necessary to reconcile it with entrenched conservation principles.

3. THE NATURE OF FUNDAMENTAL ECONOMIC THEORY

The discussion of the preceding section establishes that general equilibrium theory and game theory need not be viewed as

economic theories which are in opposition to the less elaborate specific theory typically used in neoclassical economics, but that the former theories may be considered as giving an explicit foundation for the latter type. The present section is intended to support a stronger claim, that both types of theory are essential parts of any scientific research program in economics. This claim follows from two premises: First, that any complex of specific theories which is a competitor of neoclassical applied theory must also engender a fundamental theory which would serve the functions just described for the theories of utility-maximizing agents. Second, that the complex of specific theories remains indispensable to the research program associated with the fundamental theory because the latter is not independently testable.

A general argument for the first premise will not be offered here. However, an example will be given which strongly suggests that a body of fundamental theory would arise within an alternative research program to neoclassicism. Consider a group of economists who accept the Malthusian population theory, and who are confronted with the rapid transfer of technology to third-world societies in the past several decades. This transfer indicates that groups of people who have traditionally enjoyed a low level of material welfare are willing and able to change their way of life radically in order to raise that level. This is prima facie evidence against the assumption that the working class will not curtail its birthrate to raise per capita income. In order to accommodate this evidence, the neo-Malthusians will require a fundamental theory (e.g., a detailed sociological theory of the

influence of class on aspirations and behavior) which can explain why the entire population of a third-world country is not analogous to the working-class population of a technologically developed country. The reconciliation which this fundamental theory would provide between two specific theories (i.e., those of population growth and of diffusion of technology) would be the same sort of reconciliation as Cournot's theory provided between the specific theories of monopoly and perfect competition. Thus, as in the case of neoclassical economics, fundamental theory would arise in response to the needs of specific positive theory.

It might be supposed that, regardless of its origins, fundamental theory will ultimately become autonomous and challenge the role of specific theory in directing empirical inquiry. On the basis of such an assumption, fundamental economic theory has been judged to be an unsatisfactory scientific theory. In particular, it has been argued that either fundamental economic theory is presented at so high a level of abstraction that it is irrefutable, or else that its assumptions are so oversimplified that it must be false. If it is true that fundamental theory is autonomous, this argument may well be sound. However, the argument depends crucially on the assumption of autonomy. In the methodological writings of several economists there are two proposals to refute the argument (as applied to neoclassical theory) by denying the autonomy of fundamental theory. The remainder of this section will be devoted to characterizing the relation between fundamental and specific theory on which these proposals are based.

Friedman [4] and Machlup [7] have written standard expositions

of one defense of the validity of utility maximization as a foundation for microeconomics. Both of these authors phrased their arguments in terms reminiscent of logical positivism, but that philosophical position is not essential to their point. What they suggested is that microeconomic theory is a partially interpreted theory, in which only qualitative propositions about market aggregates (i.e., price and quantity) are empirically significant. The consumers and firms whose optimizing decisions are studied in order to derive these propositions are to be regarded as purely theoretical entities, according to Friedman and Machlup, and are not to be confused with actual agents. Consequently, they claimed, neoclassical microeconomics cannot be criticized on the basis of evidence from sources such as surveys and psychological experiments, which show that actual consumers and firm managers sometimes make inefficient or inconsistent decisions.

While viewing exotic entities like quarks as fictitious may be a defensible position, it is not very persuasive to argue that the consumers and firms of economic theory bear only coincidental resemblance to actual ones. This dubious assertion is not necessary to do what Friedman and Machlup require, though. All that they need to assert is that neoclassical pure theory represents agents as maximizing utility only within a limited class of situations (i.e., those described in specific theories), and that failure of agents to maximize utility in situations outside that class (e.g., in experiments conducted in a psychology laboratory) would be irrelevant. On this view, an analogy might be drawn with the psychological theory that short-term and long-term memory are distinct mental processes. Most

laboratory experiments would be taken to deal with short-term memory, and so could not refute a theory that agents have perfect recall of material in long-term memory. Similarly, market decisions are typically made by agents whose experience and opportunity for deliberation clearly distinguish their situation from that of subjects in artificial laboratory experiments. Thus, inefficient or inconsistent behavior by experimental subjects need not be counted as evidence against the hypothesis that economic decision-makers behave optimally.

On the view that economic decision-making is a distinguished sphere of behavior, economic phenomena provide the only reliable evidence about the hypothesis that such decision-making is rational. Fortunately, the domain of economic phenomena is large and diverse. Although fundamental theory must always be tested jointly with a specific theory, there are as many distinct tests as there are areas of specific theory. Specific theories, then, serve as auxiliary hypotheses or "soft core theories" in tests of fundamental theory. Because no scientific theory is testable without such intermediation, pure economic theory is not in an exceptional position. In fact, fundamental economic theory may be particularly well situated because of the variety of tests offered by diverse specific theories.

Of course, the adoption of this view of the scientific status of fundamental theories does not make the immunity of neoclassical economics from psychological evidence a matter of a priori truth as does the partial-interpretation view. As Wilde [14] explains, the sphere of economic behavior might be construed to include laboratory situations. Whether this inclusion is appropriate is currently an

unsettled question. If laboratory situations are included, then the theory of how subjects act is a specific economic theory, and a rejection of the utility-maximization hypothesis on the basis of experimental evidence would be consistent with the account just put forth of how fundamental theories are tested.

A substantially different defense of neoclassical fundamental theory was set forth by von Mises [8], who conceded that the hypothesis of utility-maximization is not capable of falsification, but who claimed that the theorists' knowledge that economic agents are utility maximizers is a priori and that concern about its falsifiability is inappropriate. A reasonable interpretation of these claims would be that fundamental theory is a logical meta-theory of specific theory, or that it is an explicit "heuristic" of the scientific research program to which it belongs. On this view, the beneficial effects of fundamental theory discussed in the last section are not accidental "spill-over" effects, but rather they constitute its essential scientific purpose. Fundamental theory is not really a theory at all, but instead it is a set of rules and recommendations about the construction and interpretation of applied theories. If the application of these rules and recommendations is fruitful, then fundamental theory is successful.

It is evident that both of the preceding accounts of fundamental theory presuppose the existence of a closely related body of specific theory. Although proponents of the two accounts might be seriously divided on other issues, they would agree that the critics of fundamental theory are attacking a straw man. Fundamental theory

simply does not exist as a self-contained body of putative scientific knowledge.

4. SUMMARY AND CONCLUSIONS

Terms like "competing research program" and "problem-shift" have sometimes been used to describe the emergence of an explicit body of fundamental theory alongside the specific theory of neoclassical economics. Such descriptions imply that fundamental and specific theory conflict or are incommensurable. The argument of the present paper began with a detailed account of some of the close relations between fundamental and specific neoclassical economic theory. The existence of these relations indicates that conflict or incommensurability between the two types of theory is a much less serious problem than might have been thought, and it suggests that a different description of how the two types are related might be appropriate.

The argument continued by suggesting that the emergence of fundamental theory is a characteristic event in the evolution of research programs in economics, rather than being a peculiar event in the history of neoclassical economics. The description is clearly consistent with the occurrence of the kinds of interactions between fundamental and specific theory which were already noted. Furthermore, the account of fundamental economic theory given here facilitates a clear statement of the replies of several prominent economists to objections raised against it. Specifically, the account enables these replies to be stated without appeal to idiosyncratic or contested philosophical theories of knowledge and meaning. While the arguments

considered were originally put forth in terms of the neoclassical theory of utility maximization, they have been shown here to be instances of general views about the relation between fundamental and specific theory in any economic research program. While they differ on what the role of fundamental theory is, each of these general views is clearly analogous to a standard account of the nature of physical sciences.

While it is hoped that the present discussion has been sufficiently detailed to inspire confidence that fundamental economic theory is a legitimate scientific endeavor, obviously it has been far from complete. To begin with, each of the examples given in section two deserves a much more detailed examination than it has received. Also, it would be of interest to have corresponding examples from other economic research programs besides the neoclassical one. A striking feature of the definition of fundamental theory used here, having implications which deserve careful attention, is that it places far more emphasis on institutions than is traditional. An explicit theory of the domain of economic phenomena and of economic evidence is needed if the view that fundamental theories are tested via specific theories is to be considered seriously. The view of von Mises also raises questions, for instance, about how the deductive arguments of fundamental theories contribute to their usefulness as prescriptions. Work on problems such as these should lead to a clearer understanding of the structure of economic reasoning, and of the relation between economics and the other sciences.

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ON THE USE OF LABORATORY EXPERIMENTS IN ECONOMICS

Louis L. Wilde

Twenty years ago the focus of welfare economics began to shift from comparing allocations in a given economic environment to comparing systems of economic organization which might operate within entire classes of economic environments. This "$(new)^2$ welfare economics," to use Reiter's [1977] term, is not simply concerned with the outcome of a given system, but rather is concerned also with practical features of systems such as administrative feasibility, computational complexity, and cost of operation. Indeed the entire "Public Choice" approach to welfare economics is a manifestation of this shift in emphasis. Reiter discusses a number of formal examples of this kind of work, including the well-known formulation by Ted Groves and John Ledyard of a government allocation-taxation system for the optimal provision of public goods [Groves and Ledyard, 1977].

In spite of its focus on "real-world" considerations, most of the $(new)^2$ welfare economics has been theoretical. There are several reasons for this. First, the initial conceptualization of the problem was due to theorists. Their work has been very abstract and demands of the reader a substantial degree of mathematical sophistication. It has, therefore, not been generally accessible to either applied economists or decisionmakers who might be in a position to make use of the theoretical results. Second, empirical work has been difficult in this area. Often the institutions of interest do not exist and, in cases where they do, data which might allow theoretical results to be tested is hard to obtain. Finally, many of the theoretical results do not make predictions but rather suggest how institutions with desirable features ought to be designed. The needed nontheoretical work is therefore more along the lines of implementation than testing.

One possible solution to the absence of nontheoretical work in the $(new)^2$ welfare economics is nonlaboratory experiments. However, this solution has difficulties of its own. Nonlaboratory experiments tend to be very expensive, often prohibitvely so. They also require the cooperation of individuals whose major priority is making decisions, not contributing to economics at a basic scientific level.[1]

Another possible solution to the problem, and the one which is of primary interest here, is laboratory experiments. The use of laboratory experimental techniques in economics has increased dramatically in recent years, and virtually all of the major economics journals have begun to publish the results of laboratory experiments. This increased interest has been a direct response to the absence of nontheoretical work in the $(new)^2$ welfare economics.

The purpose of this paper is to review, and develop when needed, needed, the foundations of the use of laboratory experimental techniques in economics (as propounded by economists). This task will be undertaken in the next section of the paper. The discussion there will be based on the work of Charles Plott and Vernon Smith,

although some modifications of their "precepts" and "axioms" will be necessary. A third section will briefly discuss limitations of laboratory experiments.[2]

2. A PRECEPTUAL FOUNDATION

In the introduction it was suggested that the use of laboratory experimental techniques in economics has arisen in response to the need for a nontheoretical (new)^2 welfare economics. One reason for taking this point of view is that even though economists are interested in both individuals and institutions, systematic rationalizations of the use of laboratory experiments by economists have been developed only for the latter. Furthermore, a number of recent laboratory experiments have been designed to study the implementation of particular theoretical systems. For example, Smith [1975] and Ferejohn, Forsythe and Noll [1979] both analyze practical aspects of decentralized decisionmaking systems for public goods such as those formulated by Groves and Ledyard [1977]. In spite of this work, thought on the subject is still in its infancy and is being continually refined as practical experience accumulates. In fact, the "literature" consists primarily of two papers, one by Vernon Smith [1977] and the other by Charles Plott [1978]. This paper will draw heavily on the work of Plott and Smith. However, several significant modifications of the foundations offered by those authors will be introduced.

The basic idea behind a laboratory experiment in economics is to create a small-scale microeconomic environment in the laboratory

where adequate control can be maintained and accurate measurement of relevant variables guaranteed. The definition of a microeconomic environment which I will use starts with Reiter's description of an "economic environment." An economic environment consists of a list of agents $\{1,\ldots,n\}$ and commodities $\{1,\ldots,\ell\}$. Each agent is described by a preference relation $\underset{\sim}{\alpha}_i$ (represented generally by a utility function u^i), a technology T^i (represented generally by a production possibilities set) and an initial endowment vector, ω^i. The i^{th} agent is thus described by a triple, $e^i = (\underset{\sim}{\alpha}_i, T^i, \omega^i)$. Commodity space is taken to be $\mathbb{R}^\ell$. Given that the list of commodities is fixed, the economic environment is then described by $e = (e^1,\ldots,e^n)$.

To complete a microeconomic environment one needs to specify the institutional setting. As a theoretical matter, this is the focus of the exercise. For our purposes, a very simplified formalization will be adequate. Let individual i have the opportunity to make a decision $d_i \in D_i$. Each individual is assumed to select d_i so as to maximize u^i (note that externalities are not ruled out). An institution is defined by the collection of sets $D = D_1 \times \ldots \times D_n$ and a mapping $I: D \to \mathbb{R}^{\ell n}$ which takes decisions into final allocations. A microeconomic environment is then described by $E = (e,I)$.

The essential features of the above construction are as follows. First, a microeconomic environment is composed primarily of two elements, a collection of individuals and an institutional setting. Second, two properties characterize the individuals; they are assumed to possess consistent preferences and to make decisions so as to maximize their own well-being. Third, these decisions act through the institutional setting in order to determine final outcomes.

A laboratory experiment in economics attempts to create and study a small-scale microeconomic environment. Its purpose is to uncover systematic relationships between individual preferences, institutional parameters, and outcomes.[3] To realize this purpose, the experimentor must have control over both the preferences of the individuals participating in the experiment and the institutional parameters which govern final allocations. Thus, the initial tasks of the experimentor are to ensure that a genuine microeconomic environment has been created, and to ensure that enough control over individual preferences and institutional parameters can be maintained to uncover any systematic relationships between them and final outcomes. It is my aim here to find a set of sufficient conditions which, if satisfied, will guarantee that these requirements have been satisfied.

Consider first the control of individual preferences. Sufficient conditions for such control are based on the theory of "induced value" as developed by Smith [1976, 1977]. Smith's theory is implemented by mapping final allocations into a reward structure. If the reward structure satisfies certain properties (detailed below) then adequate control over preferences can be guaranteed.

Smith identifies three "precepts" as constituting a foundation for the use of laboratory experiments in the study of resource allocation mechanisms. These are nonsatiation, complexity, and parallelism. The term precept is adopted here directly from Smith. Apparently he uses it to suggest that his three conditions are properties only intended to connote general rules of action related to proper experimental design. They are thus not to be regarded as

self-evident truths (as the term axiom might imply) or as strictly sufficient conditions for a valid experiment. Indeed, Smith never states in any general way the objectives which govern the design of laboratory experiments, so that it is impossible to interpret the precepts as sufficient conditions for a "valid" experiment. Nevertheless, they provide an extremely useful starting point for developing such a set of sufficient conditions.

According to Smith, nonsatiation requires that "given a costless choice between two alternatives, identical except that the first yields more of the reward medium (e.g. currency) than the second, the first will always be chosen (preferred) over the second, by an autonomous individual."[4] Complexity recognizes that "in general" individual decisionmakers must be assumed to have multidimensional values which attach nonmonetary subjective cost or value to (1) the process of making and executing individual or group decisions, (2) the end result of such decisions, and (3) the rewards (and perhaps behavior) of other individuals involved in the decision process."[5] Parallelism asserts that "propositions about the qualitative behavior of individuals and of markets and other resource allocation mechanisms that have been tested in laboratory experiments apply also to nonlaboratory environments where similar ceteris paribus conditions prevail."[6]

Smith's precepts are intended to guarantee that a well-defined microeconomic environment has been created in the laboratory, that adequate control over that environment can be maintained, and that any "results" obtained are relevant outside the laboratory.

Consider the first of these goals. A well-defined microeconomic environment requires that individuals have consistent preferences and act so as to maximize their own well-being. Nonsatiation guarantees that both these requirements will be satisfied. However, a well-defined microeconomic environment also requires that individual decisions act through an institutional setting in order to determine final allocations which, in turn, determine rewards. In other words, rewards earned by individuals must be tied to their decisions. None of Smith's precepts guarantee that this requirement is satisfied. Thus a fourth precept, which I will call saliency, must be added to Smith's precepts. Saliency requires that the reward earned by an individual is tied to decisions made by that individual.

Together saliency and nonsatiation guarantee that the requirements of a well-defined microeconomic environment are satisfied. Saliency implies that the amount of the reward medium earned is linked to the decisions made by the subjects and nonsatiation implies that the amount of the reward medium earned is always important to the subjects. Thus a reward structure which satisfies saliency and nonsatiation can also be used to control preferences systematically. An example of a particular experiment will make this clear.

Consider an experimentor who wishes to study repeated oral double auction markets. An oral double auction is a very simple institution. A single homogeneous good is to be bought and sold. There are two types of agents, buyers and sellers. All buyers can make bids and all sellers can make offers. These bids and offers are displayed for all agents to see. If a bid is made by a buyer which

is acceptable to a seller, that seller simply accepts the bid and a transaction is recorded. Smiliarly, if an offer is made by a seller which is acceptable to a buyer, that buyer simply accepts the offer and a transaction is recorded. All agents observe the prices at which transactions are made. In any given market period, a fixed demand curve and fixed supply curve reflect agents' preferences. Over time, the same basic market is repeated, with the time path of transaction prices the focus of analysis.

In order to study this institution experimentally, it is necessary to "induce" preferences which will generate desired market demand and supply curves. On the demand side, subjects are given a table listing monetary rewards to be provided by the experimentor for units of the commodity bought. The subjects then earn the difference between the total "redemption value" of the units they have bought and the costs incurred in their purchase. As long as the subjects prefer more money to less, the redemption values specified by the experimentor constitute a well-defined demand curve. Similarly, on the supply side, subjects are given a table listing monetary costs to be assessed by the experimentor for units of the commodity sold. The subjects then earn the difference between the total revenue they have collected from sales and the total cost of the units they have sold. Again, as long as the subjects prefer more money to less, the costs specified by the experimentor constitute a well-defined supply curve. Shifts in the demand curve can be accomplished by changing the redemption values. Shifts in the supply curve can be accomplished by changing the initial costs. Moreover,

the same demand curve or supply curve can be induced on different subject pools by using identical redemption values or initial costs.

Consider nonsatiation and saliency in terms of this example. Nonsatiation is satisfied by the reward structure because it is reasonable to assume that subjects prefer more money to less. Saliency is satisfied by the reward structure because buyers earn the difference between the redemption value and the sale price while sellers earn the difference between the sale price and the initial cost. Hence buyers have an incentive to minimize the sale price and sellers have an incentive to maximize it.

Using this process of inducing values to control preferences is not without its difficulties. Problems can arise from two distinct sources. First, subjects may place additional subjective valuations on their participation in the experiment, over and above any direct payoff they receive in terms of the reward medium. Some subjects may enjoy making the calculations and decisions required of them by the experiment, while others may find such activity arduous. Second, subjects may also place subjective valuations on the rewards earned by other participants in the experiment. For example, equity may be of great concern to some subjects. The former effect is usually controlled by using a reward structure with high payoffs. I will refer to this as <u>dominance</u> of the reward structure. The latter effect is usually controlled (when appropriate) by keeping subjects uninformed of the overall pattern of the reward structure across individuals. I will refer to this as <u>privacy</u> of the reward structure. If the reward structure satisfies dominance and privacy

then Smith's precept of complexity, which is stated more as a cautionary warning than as a rule of action, will have been rendered moot.

Consider again the oral double auction market described above. Dominance is satisfied by the reward structure because a "commission" is usually paid on all transactions (this ensures that marginal units will be traded) and privacy is satisfied by the reward structure because agents see only bids and offers, not redemption values and initial costs (often they are isolated at computer terminals and thus never come into contact with each other).

Suppose now that the reward structure satisfies saliency, nonsatiation, dominance, and privacy. Then, in fact, the laboratory experiment constitutes a small-scale microeconomic environment in which real economic agents make real economic decisions. Moreover, these decisions are based on values controlled by the experimentor. Control is crucial because it is necessary for measurement and thus replicatability. Replicatability, in turn, allows the experimentor to identify <u>systematic</u> relationships between preferences, institutional parameters and outcomes. These systematic relationships constitute the "results" of laboratory experiments in economics. In other words, any systematic relationship between preferences, institutional parameters, and outcomes which has been identified by replication is <u>by definition</u> a result. The final question is, of what use are these results?

To date three distinct uses of laboratory experimental techniques in economics have been discussed. They are: (a) the appropriateness of competing theories can be distinguished; (b)

theories which are clearly nonsense can be exposed; and (c) experience can be obtained with new modes of organization. These three cases have been analyzed in depth by Plott [1978] where several examples of each are provided. Herein only a cursory discussion will be offered.

The use of laboratory experiments to distinguish between competing theories is perhaps the most classic. As Smith puts it, "the best experiment is the crucial experiment whose outcome clearly distinguishes between competing theories."[7] The problem, of course, is that the conditions which define the crucial experiment rarely occur naturally. But they can often be created in the laboratory.

It is important to note here that a crucial experiment need not be "realistic" in the sense that nonlaboratory experiments are. It only needs to include those parameters relevant to the theories which are to be tested. If it does (assuming that saliency, nonsatiation, dominance, and privacy are satisfied by the reward structure) then any failure to distinguish between competing theories is the fault of the theories, not the experiment.

This point is valid with respect to the use of laboratory experiments to reject some theories as nonsense, too. That is, if an experiment includes all parameters relevant to a particular theory, and if the theory fails to predict well in the simplified setting of the laboratory, then it cannot be expected to predict well in more complex environments. Again, the only requirements needed to reach this conclusion are that saliency, nonsatiation, dominance, and privacy are satisfied by the reward structure. The experiment does

not need to be "realistic" and no presumptions need be made about its connection to more complex ("real-world") environments.

The third use of laboratory experiments, to gain experience with new modes of organization is different in this regard. Instead of starting with an existing mode of organization and trying to develop and test theories related to it, the researcher starts with a theoretical concept which is "devoid of operational detail."[8] The task is to construct a new mode of organization which exhibits the desired performance features. Plott has referred to this as "institutional engineering."[9] It is precisely the nontheoretical $(new)^2$ welfare economics discussed in the introduction of this paper. For this use of laboratory experiments to be valid, more than saliency, nonsatiation, dominance, and privacy of the reward structure is required. There must be a link to nonlaboratory environments. This is where Smith's precept of parallelism becomes important.

Recall that parallelism asserts that "propositions about the qualitative behavior of individuals and of markets and other resource allocation mechanisms that have been tested in laboratory experiments apply also to nonlaboratory environments where similar ceteris paribus conditions prevail."[10] In terms of the definitions introduced above, this precept might be rephrased as asserting that the results of laboratory experiments apply to any microeconomic environment where similar ceteris paribus conditions prevail. The reasons for this rephrasing are twofold. First, "propositions about the qualitative behavior of individuals and of markets and other resource allocation mechanisms that have been tested in laboratory

experiments" are generally statements concerning relationships between preferences, institutional parameters, and outcomes. The only such statements which are valid are those which can be replicated. Such statements, however, constitute the "results" of any experiment. Secondly, the distinction between laboratory and nonlaboratory environments is irrelevant. What is important is that the results apply to any microeconomic environment where similar ceteris paribus conditions prevail. Here it is important to recognize that "similar ceteris paribus conditions" prevailing does not mean identical environments. It means in part that parameters affecting preferences or institutional structure are held constant except those relating to the behavioral propositions under study. These constant parameters need not be identical.

Parallelism is crucial to justifying the third, and potentially most fruitful, use of laboratory experimental techniques in economics listed above. As such, it is by no means noncontroversial. In fact, a major criticism of laboratory experiments is that they are unrealistic. That is, individuals and institutions in the real-world are claimed to be much more complex than their laboratory counterparts, and therefore any results obtained through laboratory experiments have no relevance to real-world behavior. Such criticisms either attack the theory of induced value as embodied in the precepts of saliency, nonsatiation, dominance and privacy or they attack the precept of parallelism. Smith offers two responses to the latter.

> "First, if the purpose of the experiment is to test a theory, are the elements of alleged unrealism in the experiment

> parameters of the theory? If not then the criticism must be directed to the theory as much as to the experiment. Laboratory experiments are normally as "rich" as the theories they test. Secondly, are there field data to support the criticism, i.e. data suggesting that there may be differences between laboratory and field behavior. If not, then the criticism is pure speculation; if yes, then it is important to parameterize the theory to include the behavior in question."[11]

Attacks on the theory of induced valued are primarily questions of methodology since the theory itself is internally consistent.[12] But in this case replication is the key. If replicable relationships between preferences, institutional parameters, and outcomes have been identified, then the researcher must extend or modify existing theory to explain the relationships. After all, it is only necessary that saliency and nonsatiation be satisfied in order that a microeconomic environment exist. Everything else is simply a matter of control and measurement.

The modifications of Smith's preceptual foundation introduced above were necessary to ensure that a genuine microeconomic environment was created and to ensure that enough control over individual preferences and institutional parameters could be maintained to uncover any systematic relationships between them and outcomes. One final problem with Smith's preceptual foundation is that the concept of "similar ceteris paribus conditions" is not defined in a precise way. It is here that Plott [1978] becomes particularly relevant.

As in this paper, Plott starts with Smith's theory of induced value, introducing two additional "axioms."[13] Axiom one is that "the

relationship between outcomes, preferences and institutions are (supposed to be) independent of the social alternatives."[15]

Axiom one essentially asserts that relationships between preferences, institutional parameters, and outcomes are independent of the sources of preferences. To paraphrase Plott's example, it is irrelevant whether a person is willing to pay ten dollars maximum for a shirt because (a) he or she thinks it is pretty, (b) a friend thinks it is pretty, or (c) it tastes good. In each case, that person's contribution to the total market demand for shirts is the same and, hence, so is the equilibrium price of shirts.

Axiom two essentially asserts that "labels" don't matter. Thus a competitive market for wheat should exhibit the same qualitative properties as a competitive market for health services.

These two axioms are closely related to parallelism. In particular, they are necessary for parallelism but not sufficient. In essence, they begin to define "similar ceteris paribus conditions" in a precise way. Ultimately, we would like to find a set of necessary and sufficient conditions which fully characterize the concept of ceteris paribus in the context of parallelism. Whether such a set of "axioms" can be found is an unresolved issue. Nevertheless the notion of ceteris paribus is crucial to all of economics, and thus attempts to find such a set of "axioms" is of benefit to the entire profession, not just experimentalists.[16]

3. LIMITATIONS OF THE USE OF LABORATORY EXPERIMENTAL TECHNIQUES IN ECONOMICS

So far in this paper, a set of sufficient conditions has been identified which, if satisfied, guarantee that laboratory experimental techniques are appropriate nontheoretical tools for economists. Nonsatiation and saliency of a reward structure ensure that a well-defined microeconomic environment has been created, dominance and privacy of a reward structure ensure that enough control over individual preferences and institutional parameters can be maintained to uncover any systematic relationships between them and outcomes, and parallelism ensures that any results obtained are useful outside the laboratory. Nothing yet has been said about when these conditions are likely to be satisfied.

It is not my intention to draw specific conclusions regarding this matter, but rather to make the point that criticisms of laboratory experiments must focus on precisely this issue. Section 2 of this paper indicates that laboratory experimental techniques can be valid tools for economists. The relevant questions are whether the sufficient conditions identified in this paper are ever likely to be satisfied in general or, more importantly, whether they are in fact satisfied in particular cases. An argument that they are not likely to be satisfied in general seems implausible, but there are undoubtedly examples of particular cases in which they fail.

Economists have gained experience with a wide spectrum of experiments in recent years, from relatively simple bargaining experiments [Roth and Malout, 1979], to the oral double auctions

described in the last section,[17] to relatively complicated market experiments such as Hong and Plott [1977] (which analyzed the implications of rate filing for the domestic dry bulk transportation industry on inland U.S. waterways) and Plott and Wilde [1979] (which analyzed markets for professional diagnosis and services). These experiments are listed seemingly in order of those most likely to satisfy the sufficient conditions of section 2 to those least likely to do so, given the present level of understanding. This assertion is premature, however, because laboratory experimental techniques are new to economists. They do hold much promise, though, and such strong judgments should really be suspended until more research has been done.

FOOTNOTES

*I would like to thank Ed Green, Charles Plott, Alan Schwartz, and Vernon Smith for helpful comments on the first draft of this paper.

1. Nonlaboratory experiments include token economies as well as "real-world" economies. For a review of the former see Tarr [1976], for an example of the latter see Manning, Mitchell and Acton [1976].

2. Animals have been used as subjects in some laboratory experiments by economists. Issues related to the use of such experiments in economics go well beyond this paper. See Kagel et. al [1975] for examples of this type of research.

3. This statement is adopted from Plott's "fundamental equation" (Plott, [1978]). It is nevertheless controversial. Uses of laboratory experimental techniques will be discussed in more detail later in this paper.

4. Vernon Smith, (1977), p. 3.

5. Ibid., p. 5.

6. Ibid., p. 7.

7. Ibid., p. 8.

8. Charles Plott, (1978), p. 36.

9. The choice of this label was certainly not accidential. Notice the relationship between the use of labroatory experiments and "demonstration" projects in large-scale R & D.

10. Smith, (1977), p. 7.

11. Ibid., p. 8.

12. One point of this paper is that the "theory" of induced preferences is not really a theory at all.

13. The term "axiom" is Plott's. He apparently uses it because these properties are true of all economic models.

14. Plott, (1978), p. 3.

15. Ibid., p. 6.

16. In commenting on an earlier draft of this paper, Vernon Smith also pointed out the need to articulate what ceteris paribus

might mean in the context of parallelism. This is a complicated and important issue, but not one I intend to resolve in this paper.

17. This is perhaps the best understood experimental institution. A number of variants of the basic oral double auction have also been studied (e.g. Isaac and Plott, [1978]) and some sophisticated theoretical work has also been done in response to this work (Easley and Ledyard, [1979]).

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